DOCUMENT RESUME

ED 216 379 CS 206 911

AUTHOR Tallman, John Gary
TITLE Paradigms and Problems: Thomas Kuhn and "Composition Revolutions".
PUB DATE Mar 82
NOTE 15p.; Paper presented at the Annual Meeting of the Conference on College Composition and Communication (33rd, San Francisco, CA, March 18-20, 1982).

EDRS PRICE MF01/PC01 Plus Postage.
DESCRIPTORS *Educational Theories; Higher Education; Models; *Research Design; Research Needs; *Research Problems; *Teaching Methods; *Writing Instruction; *Writing Research
IDENTIFIERS *Kuhn (Thomas S); Process Product Research

ABSTRACT

The arguments for and against Thomas Kuhn's paradigmatic thesis are presented in this paper as a means of discussing the appropriateness of Kuhn's thesis to the emerging composition discipline. Section one of the paper introduces the topic, noting how the term paradigm is used in composition to define a system of widely shared values, beliefs, and methods that determines the nature and conduct of the discipline. Section two discusses the ways critics have attacked Kuhn's thesis, focusing on four major problems with Kuhn's notions of paradigms, how they work, and how they replace one another. Section three considers the possible operations of paradigms within composition research and pedagogy, especially as the process-oriented paradigm has emerged to overtake the current-traditional (product-oriented) paradigm of composition theory. (RL)

****
* Reproductions supplied by EDRS are the best that can be made *
* from the original document. *
****

U.S. DEPARTMENT OF EDUCATION
NATIONAL INSTITUTE OF EDUCATION
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)
This document has been reproduced as received from the person or organization originating it.
Minor changes have been made to improve reproduction quality.
Points of view or opinions stated in this document do not necessarily represent official NIE position or policy.

ED216379

PARADIGMS AND PROBLEMS: THOMAS KUHN AND "COMPOSITION REVOLUTIONS"

John Gary Tallman
Associate Professor of Biology
Pepperdine University
Malibu, California

"PERMISSION TO REPRODUCE THIS MATERIAL HAS BEEN GRANTED BY
John Gary Tallman
TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)."

> The joint building activity that consumes most of our energy and binds us together is, of course, language, but this is so overwhelming a structure and grows so slowly that none of us can feel a personal sense of participating in the work.
>
> --Lewis Thomas, The Lives of a Cell

I

In his 1979 article, "Paradigms and Problems: Needed Research in Rhetorical Invention," Richard Young argues that

> . . . since the beginning of the century, the teaching and researching of composition have been guided by what Thomas Kuhn (1970) has called a "paradigm," a system of widely shared values, beliefs and methods that determines the nature and conduct of the discipline. (p. 29)

Here Young is evoking the "authority" of Thomas Kuhn and his inquiries into the epistemology of scientific discovery as a means of exploring the status of the emerging composition discipline. In his article, Young will go on to contrast two distinct "paradigms":

CS206911

(1) the current-traditional paradigm, one which Young finds impoverished because it emphasizes "the analysis of written products" and the "art of editing," excluding the classical rhetorician's important category of invention; and (2) a process-oriented paradigm which sees rhetoric as heuristic and dynamic, and which focuses attention on invention. Young, like James Kinneavy (1971) and Patricia Bizzell (1979) and a host of other researchers and theorists in the composition field, has found in Kuhn's formulations a convenient conceptualization for understanding and "taxonomizing" the evolution of composition theory. There are, however, some unvoiced assumptions behind this use of Kuhn that perhaps need exposition and analysis--an exposition and analysis I propose to offer in this paper.

Scholars within the humanities have a tendency to appropriate the models and methodologies of science with little regard to the epistemology behind them within empirical research. The humanities scholar, usually untrained in the use (and, therefore, the limitations) of such instruments and conceptualizations, may wield them uncritically. In ignorance, he may unwittingly claim too much from too little, thus undermining his own work while misconstruing the work of others. The widespread and superficial use of Thomas Kuhn's twenty-year-old paradigmatic thesis outside of scientific fields is problematic for several reasons, but especially so, it seems to me, for those in the humanities who may have been oblivious to the long-standing debate among scientists and

philosophers of science over the validity of Kuhn's thesis.

Shortly, I wish to examine Kuhn's thesis a bit more carefully, suggesting the ways his critics have attacked it, and to offer some personal observations regarding the possible operations of "paradigms" within composition research. But before that, let me state succinctly that there are two major stumbling blocks in dealing with Kuhn's paradigmatic thesis: (1) first, those who have appropriated this paradigmatic notion from Kuhn, especially outside of the hard sciences, seem to have greatly misunderstood Kuhn himself; and (2) secondly, since its appearance, Kuhn's thesis has been met with a steady and quite vocal criticism which has challenged almost every point in Kuhn's original formulation. More about this later, but it should be said here that Kuhn's thesis and other Weltanschauungen views (such as those of Paul Feyerabend and Stephen Toulmin) are regarded within the philosophy of science with such disdain that Frederick Suppe, in his 1977 afterword to his massive The Structure of Scientific Theories, could write "The Weltanschauungen views, in a word, today are passé, although some of their authors continue to develop them and they continue to be much discussed in the philosophical literature" (pp. 633-34).

II

Immediately after Kuhn published his The Structure of Scientific Revolutions in 1962, there came a barrage of criticism

attacking his rather ambiguous use of the word "paradigm." Margaret Masterman, herself a Kuhnian supporter, counted twenty-one (21) different senses of the word "paradigm" in that work (1970). Kuhn acknowledged this unfortunate ambiguity in his 1970 postscript to the original edition, emphasizing two main denotations; paradigms refer to:

(1) accepted examples of actual scientific practice--examples which include law, theory, application and instrumentation (p. 10). Kuhn refers to these examples as exemplars.

(2) models from which spring particular coherent traditions of scientific research (p. 10). Kuhn elsewhere refers to these as disciplinary matrices.

It is this second sense of "paradigm" that has received the most attention in non-scientific fields and which should receive our scrutiny here. For Kuhn, paradigms as "disciplinary matrices" have two essential characteristics:

(1) they are "sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity" and

(2) they are "sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve" (p. 10).

Kuhn further argued that major scientific revolutions occur when existing "paradigms" can no longer be articulated and modified to the satisfaction of the community practicing what he calls "normal

science." Normal science involves scientists dutifully going about their business within the accepted paradigm, trying--in other words--to make data "fit" with existing notions and research programmes. Kuhn juxtaposes "normal science" with "extraordinary science," in which unprecedented experimentation and conceptualization lead the scientist to overthrow previous assumptions. It is at this point, Kuhn argued, that scientific progress occurs--when the existing paradigm is discarded and a new paradigm which better satisfies the community is accepted by the practitioners whom it governs. Kuhn's view of scientific progress, in a nutshell, is that science is not evolutionary, but revolutionary. Science is not for Kuhn (as it is, say, for Toulmin), the gradual approximation of reality or the pursuit, per se, of "truth." Science is instead practiced by a group of "puzzle-solvers" (and not problem-solvers) who are seeking to resolve difficulties that ordinarily confront them as individuals, not as difficulties in theory as such. Such puzzles are challenges to the scientist's personal ingenuity and not to his sense of an inadequate theoretical stance, which is, of course, received as a given by the scientist. That a paradigm shift occurs, then, is the result of an extraordinary turn of events or conceptualization and not because of the steady progress within the day-to-day work of the laboratory scientist. Thus, Einstein and his radical break with Newtonian physics.

The limits of time prevent me from recounting all of the critical responses that have been made to the Kuhnian thesis, and

not all of them, perhaps, would be immediately intelligible to a diverse audience such as this without a great deal of elaboration. But let me suggest four major problems that have been set forth by Kuhn's critics.

First, there is the lingering problem regarding Kuhn's notion of the incommensurability of paradigms, i.e., that

> observational data and criteria for assessing theories are paradigm-dependent. . . . A shift of paradigms during a scientific revolution is not a matter of logical argument, but of "conversion." (Barbour, 1974, p. 93)

Many have noted Kuhn's consistent use of an analogy between religion and science on this point and have argued that Kuhn's "scientific choice" is ultimately irrational, subjective and relative to a "particular scientific community" (Barbour, p. 93).

Secondly, and as a corollary to the first criticism, Kuhn's notion of incommensurability logically leads to what John Watkins has called "the Paradigm Monopoly" thesis:

> A paradigm brooks no rivals: it is built into Kuhn's concept of a paradigm that one scientist cannot, while under the sway of one paradigm, seriously entertain a rival paradigm. If he has started toying with a rival paradigm, then the old paradigm is already defunct for him. (1970, p. 34)

It becomes problematical, thus, for scientists laboring under one paradigm to understand and exchange critical views with those operating under another one--since Kuhn argues that because there is no "neutral observation language," a switch in paradigms also

involves a revolution of meaning in the very terms used under the rival paradigms.

Thirdly, these notions of incommensurability and monopolistic paradigms suggest that there is no "interregnum" period between paradigms, hence no real "pre" or "post"-paradigm situations. In Watkins' terms, Kuhn's thesis calls for a kind of "instant paradigm" (1970, p. 35). In Kuhn's own words:

> The new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of the man deeply immersed in crisis. (1970, p. 89)

These implications of Kuhn's theory have been assailed by philosophers of science such as Imre Lakatos and Karl Popper who argue that communication and "critical discussion" may always take place between competing theories or paradigms and that, although there may be some initial ambiguity in the emerging vocabulary within the new "paradigm," communication is possible.

Finally, a fourth criticism has disputed Kuhn's view of "normal science." One of the main critics here has been Sir Karl Popper who suggests that if such a thing as "normal science" exists, it does so only in a very pedestrian sense--"the not-too-critical professional . . . the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it" (Popper, 1970, p. 52). Kuhn's normal science, Popper argues, is part and parcel of "The Myth of the Framework," which is,

> in our time, the central bulwark of irrationalism. My counter-thesis is that it simply exaggerates a difficulty into an impossibility. The difficulty of discussion between people brought up in different frameworks is to be admitted. But nothing is more fruitful than such a discussion. (1970, p. 57)

These criticisms thus expressed, it is time to address the use of Kuhnian theory within the composition field and how it may have been both misleading and unhelpful in certain instances.

III

It seems apparent from my reading of the composition literature which employs some of the Kuhnian thesis that many researchers use the term "paradigm" in a rather casual way, almost as a synonym for older terms such as "perspective" or "approach" and mean nothing quite so substantial by "paradigm" as Kuhn does. As far as it goes, this is an innocuous use of the term and may simply be attributed to a cross-pollinization of jargon. Others, however, may have come to think of Kuhn's "paradigm" as what E. D. Hirsch calls "a recent, documented, scientific version of [such terms as] worldview, ideology, episteme. . ." (1976, p. 148). Hirsch suggests that Kuhn's paradigmatic theory has a structural similarity to all such epistemologies which seem to make us all "trapped inside some prison-house of the mind" (p. 148). (It may be interesting to note in this context that in response to his critics, later versions of Kuhn's theory have taken on a neo-positivistic stance--an ironic turnabout, considering that Kuhn's original thesis was begun as a

counter to positivism in science.)

It is true that Richard Young, in the article mentioned earlier, concedes the ambiguity of "paradigm" in Kuhn's book, and specifically appropriates only the notion of the "disciplinary matrix" for use in examining the field of composition. But one must ask whether it is possible to extract this one component of Kuhn's thesis profitably, given the problematic status of the overall theory. It could be argued that this appropriation is just a matter of selecting a richer, more inclusive term than, say, "perspective," and that it is largely a common-sensical notion that all researchers work within a certain tradition. This, however, is the very point at issue--whether Kuhn's paradigmatic thesis as he originally formulated it and has recently refined it can be applicable to the study of a given discipline. Kuhn has not merely argued what most reflective observers had already previously conceded, viz., that each scientist works within a specific research program. Rather, he has boldly posited that "change" or "progress" in science is not essentially a matter of <u>new knowledge</u> made possible by better, more insightful experimentation. For Kuhn, "revolution" in science is much closer to what might be called a "rhetorical process," a matter, in his words, of "conversion" more than a pragmatic recognition of the actual progress of scientific work itself.

A strict application of these Kuhnian tenets would suggest that in composition, the transition between the "current-traditional" paradigm and a "process" paradigm is <u>not</u> a matter of acquiring more compelling

data, leading to revised pedagogy, as much as a "conversion" to a new framework and new nomenclature through a rhetorical debate. But this undermines the whole enterprise of teaching and research. Presumably, teachers of composition want to know that their approaches and techniques are actually and demonstrably more effective than previous ones--not merely that within a new, subjective paradigm that these "fit" more suitably.

It is here that we must urge Karl Popper's insistence on the objective testability of paradigms and hypotheses. Indeed, in areas of science where hypotheses are not testable, little progress has been made in producing unifying paradigms. The recent resurgence of controversy surrounding the Biblical account of creation in opposition to a theory of macroevolution is one example. This has led anthropologist, Ashley Montagu, to remark

> Science has proof without any certainty. Creationists have certainty without any proof.

Kuhn seems to have conflated the inevitable process of "critical discussion" in science (which is a rhetorical function) which attends the birth of new hypotheses and experimentation with the objectivity of the experiments and the data yielded by them. It is to Richard Young's credit that he sidesteps Kuhn's evident relativism here by calling for metatheories of evaluation for emerging theories of invention in the teaching of writing (1978, 42-47).

Composition theorists who would make use of Kuhn's theory must inevitably refine and qualify it to the extent that it little resembles the rather radical stance which his The Structure of Scien-

tific Revolutions takes. We may all want to embrace theories of subjectivity for professional respectability, but in actual practice--in teaching and research--we simply cannot do without a notion of objectivity which rewards real advances and penalizes error. I do not wish to argue at this point whether "objectivity" can be proven, as much as I want to point out the consequences for science and for composition unless some kind of objectivity is posited. Let me close with some outsider's observations about the composition enterprise.

Whether a composition researcher is able to recognize that he or she operates in paradigm probably holds little relevance; likewise, it is probably equally unimportant trying to decide whether or not we are on the verge of a "revolution." It can be safely said that the average bench researcher in science probably knows little of paradigms or any other structure outside his chosen specialty. Revolutions are products of crisis--they occur, as it were, independent of recognition, and thus it is probably less important to know if we are in a revolution, than it is to know if we need a revolution. In science this judgment can and has been made because

1) each community is a collection of specialists with clearly defined and highly focused goals and testable hypotheses and theories;

2) the community has both formal and informal previously agreed-upon criteria for judging the validity and/or usefulness of competing theories;

3) the members of the community have a sense of personal involvement and excitement in "puzzle-solving" activities.

Herein may lie the most significant difference between the emerging

composition discipline and established scientific theory. As Young has suggested, the means for analyzing and teaching the composing process are only now coming into being. Nevertheless, I do not believe that composition theory is in a "pre-paradigm state," nor do I believe that extant theories of invention and the composing process should necessarily be seen as paradigms which will replace the "current-traditional" paradigm. This view would accept a form of "incommensurability"; if actual "progress" is to be made, I would argue that each of these theories must develop its own framework (and community). This may lead to more and more specialization as parts of each new framework will emerge and more specialists will be needed as parts are explored and articulated in detail. A final unifying paradigm should emerge out of these "sub-paradigms"--containing the immutable elements of the current competing theories.

Patricia Bizzell's expressed apprehension over developing a composition paradigm from empirical data (1979) seems to me unfounded. In "normal science," this process is somewhat analogous to rearranging the things in one's attic. In this rearrangement, no possesions are ever truly discarded. Instead, the most useful (and hence most frequently used) items are arranged for easiest access. Since all data is subject to re-interpretation when the circumstances warrant it, this flexibility allows for correction. In the teaching of science, extensive use is made of problems and laboratories to develop the student's puzzle-solving abilities; these are designed to teach mechanics, operations and process. Herein may lie a second dis-

tinction between composition research and science. Until the inquiries into the composing process produce a sufficient understanding of what it entails physiologically and psychologically, composition pedagogy may continue to stress mechanics and editing with little attention to process-oriented activities. It may well be that the difficulties inherent in deducing the elements of the process from written products has discouraged the progress of research in this area. Nevertheless, any unifying paradigm will almost certainly contain contributions from both product-oriented and process-oriented activities in order to articulate the manner in which a successful composition is produced.

References

Barbour, Ian. Myths, Models and Paradigms. New York: Harper and Row, 1974.

Bizzell, Patricia. "Thomas Kuhn, Scientism and English Studies," College English, 40 (March, 1979), 764-71.

Hirsch, E. D. Aims in Interpretation. Chicago: University of Chicago Press, 1976.

Kinneavy, James L. A Theory of Discourse. New York: Prentice-Hall, 1971.

Kuhn, Thomas S. The Structure of Scientific Revolutions. 2nd Edition. Chicago: University of Chicago Press, 1970.

Masterman, Margaret. "The Nature of a Paradigm." In Imre Lakatos and Alan Musgrave, Criticism and the Growth of Knowledge. Cambridge: Cambridge University Press, 1970, 59-90.

Suppe, Frederick. The Structure of Scientific Theories. 2nd Edition. Urbana: University of Illinois Press, 1977.

Popper, Sir Karl. "Normal Science and its Dangers." In Imre Lakatos and Alan Musgrave (1970) (See Masterman), 51-58.

Watkins, John W. N. "Against 'Normal Science'." In Imre Lakatos and Alan Musgrave (1970) (See Masterman), 25-38.

Young, Richard. "Paradigms and Problems: Needed Research in Rhetorical Invention." In Charles Cooper and Lee Odell, Research on Composing. Urbana: NCTE, 1978, 29-47.